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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Assessing modelled spatial distributions of ice water path using satellite data" *by* S. Eliasson et al.

S. Eliasson et al.

s.eliasson@ltu.se

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[a4paper,10pt]article

enumerate

Overall

Thank you for your valuable input, and I definitely agree that the article is lacking in the sections you have pointed out, and need to be explained much more clearly. I will answer your comments and questions systematically.

Answers to comments

Objective

With regards to stating a more precise objective of the paper and really pointing out that this study builds on earlier work from [Waliser et al. 2009], I totally agree with this, and your example on how to rephrase this is a good suggestion. We will mention more about what has been done in a more concrete fashion, especially in that much of the reasoning in our paper has its roots derived from the Waliser et al. 2009 paper.

The value of CloudSat

I mostly agree on the comment on the importance of CloudSat retrievals compared to retrievals from long term datasets. We assess the general features of the IWP spatial distribution, and primarily not the mean absolute IWP, and we argue that, as the spatial distribution of IWP from long term data sets such as PATMOS-x is less influenced by biases which may be attributed to inter-annual variability, such as ENSO events, they are therefore most suitable to address the climate distributions of IWP, which the models produce. For instance, CloudSat's temporal range is short and predominantly spans La Nina conditions, which has an impact on the spatial distribution of clouds, especially in the tropics.

In terms of absolute IWP and especially the information on the vertical structure of the clouds, CloudSat is extremely worthy and valuable, and probably provides the best estimate of this quantity. For this reason CloudSat is chosen as a reference data set, especially in comparison to the observational data sets from passive sensors, for a common time period. We will rephrase the wording here, as I in no way want to undermine the importance of the CloudSat IWP retrievals. I agree that in the absolute IWP space, the uncertainties introduced by inter annual variability is small indeed compared to the other problems, and this will be highlighted more in the paper.

Refer to cloud-ice estimate from non-precipitating clouds

I do not fully understand this method. The problem for me is that many Cirrus clouds contain sedimenting ice, that will not likely reach the ground as precipitation, whereas other Cirrus clouds can contain less of the sedimenting particles. Both cases appear to be in the same category used for these results in the Waliser et. al. paper, as neither clouds produce ground precipitation. Furthermore, how does removing precipitating clouds impact the model to observation cloud-ice comparison, as the precipitating clouds also contain cloud-ice? Will this not produce a kind of dry bias in the observations? Having said this, the results have value and should be mentioned to help explain the cloud-ice fraction (ratio) problem.

Model selection

The subset of models where chosen in an ad hoc fashion. They were chosen so that the general spread of the sub-set of models would be the same as the general spread for all the models in the archive. I will clarify this better, and mention the number of models from which the subset was chosen.

Cloud-ice fraction

I clearly see the error of my ways here. I suggest using the name cloud-ice column ratio.

CloudSat error estimate

The uncertainty refers to the estimates attained from the Heymsfield et al. 2008 paper, and is introduced earlier in section 2.2.1 about the CloudSat data in our paper. I should, of course re-refer the uncertainty estimate, when I use it in the zonal mean IWP comparison section. The grey shading used in the model plot is, in an ad hoc fashion based on:

$$\mathsf{LU}(i) = .6 \times \mathsf{CR}_{\mathsf{FvMMF}}(i) \times \overline{\mathsf{IWP}}(i)$$

where LU is the lower limit of the uncertainty range, $\mathsf{CR}_{\mathsf{FvMMF}}$ is the cloud ice fraction deduced from figure 11e in your paper, $\overline{\mathsf{IWP}}$ is the mean IWP of CloudSat, and $i = -90 : 90$. i.e the calculation is done for each 1° latitude, so e.g $\mathsf{CR}_{\mathsf{FvMMF}}(12)$ is the cloud-ice ratio at lat = 12N deduced from figure 11e. For the upper bounds I have used:

$$\mathsf{UU}(i) = 1.4 \times \mathsf{CR}_{\mathsf{RAVE}}(i) \times \overline{\mathsf{IWP}}(i)$$

where UU stands for Upper uncertainty. I hope this makes it clearer. I will explain this thoroughly in the new version. By the way, the x-axis for fig11e and f show longitudes instead of latitudes ;)

Normalization

You are spot on that there is a problem in how I am wording the results deduced from normalized IWP fields and I will rewrite this section so that it is clear when we are talking about observations in the normalized space or not. Also, I will move the statistics used in normalization out of the figures and into tables as you suggested.

Is section 3.4 related to Fig 18?

Yes, it appears so. We are comparing the data sets especially in terms of spatial and temporal correlation and the standard deviation of IWP (which is sensitive to sampling) along with the mean. Fig 18 of your paper shows the mean values of the data sets. I see now that I should reference to this figure or at least point out that the mean has been previously investigated in your paper, and that there is an agreement in the mean values between the Tab 3 in our paper and Fig 18 in your paper.

Discussion needs to be changed

This section certainly needs to be changed. I agree that I am fuzzy on the notion that models and observations can hardly be compared, hence the use of 'best agreement' and 'the models are in agreement in absolute terms' needs to be changed. We should not really state this, or at least we should word it in a way that the user does not forget the problems associated comparing models and datasets.

With our statement "good agreement in the subsidence region in absolute IWP", this is probably fairly true in that the Cirrus clouds in this region as I expect them to have a low precipitating cloud particle fraction due to their nature, i.e. often induced by gravity waves (I can not remember the paper that says this).

Small issues

I will reference to Wu et. al. 2009 as suggested.